occur in the protoplasm of the cells in the intestinal wall and liver, and of the enzymes by which these processes are in all probability carried out that we must look for an explanation of the conversion of the poisonous albumoses formed during digestion into innocuous albumins, and of dangerous enzymes into harmless zymogens.

Moreover, it seems to me that it is by researches into the nature and action of the enzymes not only of microbes, but in the various tissues of the body in higher animals, that we shall learn how the microbes, like the enzymes of the intestinal canal, produce poisonous albumoses, and how the tissues, like the cells of the intestinal walls or liver, convert them into harmless or even protective substances. In this way we may hope to obtain an explanation of toxins and anti-toxins, of pathogenesis and immunity, as well as of the nature of diseases unconnected with the presence of microbes, such as diabetes. Twenty-three years ago I attempted to obtain a glycolytic enzyme from muscle, in order to enable diabetic patients to utilise the sugar in their blood. My attempt was unsuccessful, but we may still hope that by other methods we may obtain from animal organs various enzymes, the administration of which may prove as useful in other diseases as the thyroid in myxœdema.

Practical medicine depends on physiology, pharmacology, and pathology, but all three are tending to become more and more subdivisions of the wider and all-embracing science of chemistry. It is to a chemist, Pasteur, that we owe the wonderful development of pathology within the last quarter of a century, and we may fairly regard his fellow-countryman, Lavoisier, as the founder of this science. Men from all countries, and especially from Germany, have aided its development; but it seems fitting that at this Congress, in acknowledging our obligations to this science, we should not omit to mention that at its head now stands a Russian, Mendeleef, whose marvellous prescience enabled him to predict the existence of elements which were then unknown and even to describe their properties more correctly than those who first verified his predictions by obtaining the substances themselves. When we consider that little more than a hundred years have elapsed since the time of Lavoisier, and contemplate the vast benefits which medicine and its allied sciences have derived from chemistry during this time, our hopes cannot be otherwise than great for the centuries to come.

THE BRITISH ASSOCIATION.

TORONTO, *August* 25.

THIS has not been a large, but it has been a very interesting and satisfactory meeting both from the scientific and the social point of view. Out of the 1362 persons present, an unusually large proportion were well-known representative men of science. The more prominent members of each section seem to be with us, and it has been frequently remarked during the meeting that in our sectional officers we have brought over an unusually strong team. We have a fair number of distinguished foreigners, and a considerable number of the members of the American Association: these foreign members include General Billings, Dr. Anton Dohrn of Naples, Profs. Minot, Newcomb, Osborn, Putnam, Ira Remsen, Runge (Hannover), and others. Many Canadians have also taken part in the proceedings, and it may be said that in all the sections the work has been influenced in its nature and direction by our place of meeting. Canadian surveys, geography and climate, the chemistry of the soil, the fisheries and the biology of the lakes, the plants, the timber from the engineering point of view, statistics and trade combinations all received attention; while Sections C (Geology) and H (Anthropology) may fairly be said to have been dominated by the local spirit. The president of C was a distinguished Canadian geologist, Dr. George Dawson, and although the preliminary arrangements had to be made in this case by correspondence, the work of the section was remarkably well organised, and an unusually large number of abstracts were printed. There were papers and demonstrations on the rocks of North America from the Laurentian to the Glacial deposits, and several expeditions of geological interest were organised under local leaders. The Anthropological Section had a succession of interesting papers—notably those of Miss Alice Fletcher—dealing with the folk-lore, customs, religion, &c., of the North American Indians. The work of these two sections appropriately culminated to-day in a joint discussion on "The First Traces of Man in North America," during which Prof. Putnam gave an account of the supposed remains from the Trenton gravels. Prof. Claypole spoke of the human relics in the Drift of Ohio, and Sir John Evans criticised the evidence adversely and showed the probability of error as to the occurrence of the specimens supposed to be from glacial deposits.

Amongst other notable papers which attracted public attention were Profs. Meslans, Moissan and Dewar's demonstration of the preparation and properties of fluorine, and Lord Kelvin's statement as to the fuel supply and air supply of the world.

There seemed to be comparatively few joint meetings of sections—perhaps because each section was so full of its own work. Besides the combined session of C and H, mentioned above, E and F met to hear Mr. F. C. Selous on the economic geography of Rhodesia, and sections C, I, and K joined in discussing the chemistry and structure of the cell *à propos* of Prof. Meldola's paper on the rationale of chemical synthesis, Prof. Green's paper on an alcohol-producing enzyme in yeast, Prof. Macallum's paper on the significance of intracellular structures, and some histo-chemical demonstrations by Profs. Boyce and Herdman on the presence of copper, and by Prof. Macallum on the distribution of iron, in tissue cells.

The custom seems increasing in most of the sections of having a certain number of short addresses, lecturettes, or demonstrations on subjects of novelty or current scientific interest given at fixed times, and usually illustrated by lantern slides. Amongst these at the present meeting may be mentioned:—Sir George Robertson on Kafiristan, Prof. Dixon on explosion flames, Prof. Osborn on American Tertiary mammals, Dr. Munro on the Glastonbury Lake Village, Prof. Herdman on the oyster question, Prof. Poulton on mimicry and natural selection, Prof. Haddon on the evolution of the cart and the Irish car, and Mr. Seward's address on fossil plants. Section K had also some interesting papers and discussions on hybridisation, including an account by Dr. W. Saunders, Director of the Dominion Experimental Farms, of some experiments in cross-fertilisation made with the practical object of producing improved forms of apple and other fruit trees which will stand the severe climate of the North-west Territories. As an outcome of our visits to the experimental farms, and of what we have heard of them, both in the sections and outside, an important resolution, forwarded from Section B (Chemistry), has passed the Committee of Recommendations and the General Committee, to the effect that "the Council be requested to consider the desirability of approaching the Government with the view of the establishment in Britain of experimental agricultural stations similar in character to those which are producing such satisfactory results in Canada."

It may also be stated that, as the result of the consideration given by Section D to the fauna of the lakes, it is expected that a biological station will shortly be established by the Government of Ontario. A deputation of zoologists with this object in view waited upon the Premier and other members of the Cabinet, and the proposition was very favourably received. A fuller account of this matter will be given in the report of the proceedings of Section D.

Several of the sections, recognising that we are meeting in a new country, the natural products of which are more or less unfamiliar to many of the members, organised afternoon and even whole-day excursions in the neighbourhood of Toronto, under the guidance of local leaders. In this way Section B visited the chemical works at Niagara, Section C the glacial deposits, while Sections D and K had several joint expeditions to localities of biological interest. The General Committee at its final meeting to-day passed the report of the Committee of Recommendations recommending grants to committees for scientific purposes amounting to 1350*l.*, a much larger sum than has been voted for some years. One of the reasons of this increase is that the committee are extremely anxious to make some grants for the pursuit of local investigations, to be expended by the various committees which have been appointed for the purpose of study and research in Canada. These Canadian committees are appointed for the following objects: the establishment of a meteorological observatory on Mount Royal, the investigation of the pleistocene fauna and flora, the collection of geological photographs, the biology of the lakes of Ontario, the condition of the North-west tribes, the ethnographic survey of Canada, and the establishment of a biological station in the Gulf of St. Lawrence. The total amounts voted to Committees are shown on the next page.

	£	s.	d.
A—*Mathematics and Physics.*			
*Foster, Prof. Carey.—Electrical Standards	75	0	0
*Symons, Mr. G. J.—Seismological Observations	75	0	0
*Atkinson, Dr. E.—Abstracts of Physical Papers	100	0	0
*Harley, Rev. R.—Calculation of Certain Integrals	20	0	0
*Shaw, Mr. W. N.—Electrolysis and Electro-chemistry	35	0	0
Callendar, Prof.—Meteorological Observatory at Montreal	50	0	0
B—*Chemistry.*			
*Roscoe, Sir H. E.—Wave-length Tables of the Spectra of the Elements	20	0	0
*Reynolds, Prof. J. Emerson.—Electrolysis Quantitative Analysis	12	0	0
*Thorpe, Dr. T. E.—Action of Light upon Dyed Colours	8	0	0
Evans, Sir J.—Promotion of Agriculture	5	0	0
C—*Geology.*			
*Hull, Prof. E.—Erratic Blocks	5	0	0
*Bonney, Prof. T. G.—Investigation of a Coral Reef	40	0	0
*Flower, Sir W. H.—Fauna of Singapore Caves (unexpended balance in hand, 40*l.*)	—		
*Geikie, Prof. J.—Photographs of Geological Interest	10	0	0
*Marr, Mr. J. E.—Life-zones in British Carboniferous Rocks (unexpended balance in hand)	—		
Dawkins, Prof. W. Boyd.—Remains of the Irish Elk in the Isle of Man (unexpended balance in hand)	—		
*Jamieson, Mr. T. F.—Age of Rocks near Moreseat	10	0	0
Dawson, Sir J. W.—Pleistocene Fauna and Flora in Canada	20	0	0
D—*Zoology.*			
*Herdman, Prof. W. A.—Table at the Zoological Station, Naples	100	0	0
*Bourne, Mr. G. C.—Table at the Biological Laboratory, Plymouth	20	0	0
*Flower, Sir W. H.—Index Generum et Specierum Animalium	100	0	0
Miall, Prof.—Biology of the Lakes of Ontario	75	0	0
*Herdman, Prof. W. A.—Healthy and Unhealthy Oysters	30	0	0
E—*Geography.*			
*Ravenstein, Mr. E. G.—Climatology of Tropical Africa	10	0	0
F—*Economic Science and Statistics.*			
Sidgwick, Prof. H.—State Monopolies in other Countries	15	0	0
Price, Mr. L. L.—Future Dealings in Raw Produce	10	0	0
G—*Mechanical Science.*			
*Preece, Mr. W. H.—Small Screw Gauge	20	0	0
H—*Anthropology.*			
*Tylor, Prof. E. B.—North-western Tribes of Canada	75	0	0
*Munro, Dr. R.—Lake Village at Glastonbury	37	10	0
*Brabrook, Mr. E. W.—Ethnographical Survey (and unexpended balance in hand)	25	0	0
*Evans, Mr. A. J.—Silchester Excavation	7	10	0
*Dawson, Dr. G. M.—Ethnological Survey of Canada	75	0	0
Turner, Sir W.—Anthropology and Natural History of Torres Strait	125	0	0
I—*Physiology.*			
Gaskell, Dr. W. H.—Investigation of Changes associated with the Functional Activity of Nerve Cells and their Peripheral Extensions	100	0	0
K—*Botany.*			
Farmer, Prof. J. B.—Fertilisation in Phæophyceæ	15	0	0
Corresponding Societies.			
*Meldola, Prof. R.—Preparation of Report	25	0	0
	£1350	0	0

* Reappointed.

The evening lectures given before the Association were much appreciated. The projection of Prof. Roberts-Austen's electrical furnace upon the screen was especially a source of much interest to the inhabitants of Toronto. The evening reception given by the Governor-General and Lady Aberdeen was a brilliant function. At the second conversazione, given by the Local Executive Committee, one missed the usual lantern lectures, demonstrations and exhibits; but it was a pleasant gathering marked by much cordiality between hosts and guests, and utilised by members for comparing notes as to the work of the sections, and as to plans for taking advantage of the numerous expeditions and visits to places of interest provided for their visitors by the Local Committee. Four parties start for the Pacific coast during the next few days, stopping at various points of scientific or industrial interest on the way, under the guidance of men of first-rate local knowledge such as Dr. George Dawson, Dr. W. Saunders, Mr. Walker, one of the Local Secretaries, and others. The end of this article is being written in the train as the first of these parties, including Lord Kelvin and Sir John Evans, is passing along the northern shores of Lake Superior. Other expeditions have been organised to the Muskoka Lakes, to Niagara, to Parry Sound, &c., in addition to visits to Montreal and Ottawa.

The arrangements of the Local Committee have been excellent, and it was felt at the concluding meeting, when cordial votes of thanks were passed to our hosts, to the Local Executive, and especially to Prof. Macallum and his colleagues the Local Secretaries and Treasurers, that much hard work had been done, and that our thanks were thoroughly deserved. The University of Toronto and Trinity University both conferred honorary degrees upon a few of their eminent guests, and a great banquet in honour of Lord Kelvin, Lord Lister and Sir John Evans, the three notable figures of the gathering, formed a fitting and enthusiastic termination to a memorable meeting.

W. A. HERDMAN.

SECTION K.

BOTANY.

OPENING ADDRESS[1] BY PROF. H. MARSHALL WARD, SC.D., F.R.S., F.L.S., PRESIDENT OF THE SECTION.

THERE are many industrial processes which depend more or less for their success on bacterial fermentations. The subject is young, but the little that has been discovered makes it imperative that we should go on, for not only are the results of immense importance to science, but they open up vistas of practical application, which are already being taken advantage of in commerce, and we may be sure that every economic application of such knowledge will give the people employing it an advantage over those who proceed by the old rule-of-thumb methods, where nobody knows or cares where the waste or leakage occurs that spoils a commercial product.

The discovery by Alvarez of the bacillus which converts a sterilised decoction of indigo plant into indigo sugar and indigo white, the latter then oxidising to form the valuable blue dye, whereas the sterile decoction itself, even in presence of oxygen, forms no indigo, may be cited as a case in point. It remains to be decided whether this bacillus alone is concerned, or whether the infusion of indican will ferment under the action of enzymes alone derived from the leaves of the indigo plant. It also remains for future investigation to determine whether the indigo bacillus is the same as the pneumonia bacillus—which resembles it—and will also induce the indigo fermentation, and to explain why the woad-makers of the Fens find a sale for this indigo preparation among indigo makers, as well as to clear up certain mysterious "diseases" in the indigo-vats. Our much more extensive knowledge of the diseases of beer and wine suggests the possibility of profitable bacteriological investigations in several directions here.

That certain stages in the preparation of tobacco-leaves—as also in the preparation of tea—depend on a carefully regulated fermentation, which must be stopped at the right moment, or the product is impaired, or even ruined, has long been known. Regarding the possible *rôle* of bacteria in the preparation of tea, nothing is ascertained, but, if Suchsland's investigations are confirmed, there is among the many and various organisms concerned in the fermentation of West Indian tobacco a bacterium which has been isolated and plays an important part. It is claimed that the flavour of European-grown tobacco can be materially improved by its use. I read that the process is patented, which may or may not affect its value as a scientific announcement; but in view of the increasing number of researches into this subject by Behrens, Dávalos, Schloesing, and others, it is evidently a domain for further bacteriological investigations in a properly equipped laboratory.

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[1] Concluded from p. 461.

Every botanist knows that flax and hemp are the bast fibres of *Linum* and *Cannabis* respectively, separated by steeping in water until the middle lamella is destroyed and the fibres isolated; but it is perhaps not so well known that not every water is suitable for this "retting" or steeping process, and for a long time this was as much a mystery as why some waters are better than others for brewing.

Only quite recently Fribes, working under Winogradsky, has isolated the bacillus which accomplishes this dissolution of the middle lamella, and its behaviour brings to light some very interesting details, and furnishes another of those cases where the reactions of living micro-organisms can be utilised in deciding questions of plant chemistry too subtle for testing with ordinary reagents.

You are aware that recent researches, especially those of Maquin in France and of Walter Gardiner in Cambridge, Cross and Bevan and others, have caused us to discard the view that the middle lamella is composed of cellulose, and to learn that it consists of pectin compounds. Now Fribes' anaërobic bacillus dissolves and destroys pectins and pectinates, but does not touch cellulose or gum, and thus enables us to criticise from a new point of view the bacillus (*B. Amylobacter*) which Van Tieghem asserted to be the cause of cellulose fermentation and the retting of flax. Clearly it cannot be both, otherwise the flax-fibre would be destroyed; and we know from other facts that *B. Amylobacter* is not the cellulose ferment.

Fribes' discovery has yet to be tested with reference to other processes of retting. The Indian Government have lately published a series of notes on jute and other fibres, and the description of the retting of jute suggests this as a very definite problem for investigation.

I am told that a patent exists in the United States for a process whereby the retting organisms may be sown and encouraged in waters otherwise unfitted for the steeping of flax, &c., another indication of the keen interest taken in these matters.

It goes without saying that the steeping of skins in water in preparation for tanning involves bacterial actions, owing to which the hair and epidermal coverings are removed; but it appears from recent investigations that in the process of swelling the limed skins, the gases evolved in the substance of the tissues, and the evolution of which causes the swelling and loosens the fibre so that the tanning solutions may penetrate, are due to a particular fermentation, caused by a bacterium which, according to Wood and Wilcox, is similar to, if not identical with, a lactic ferment. If Haenlein's results may be accepted, it is a bacillus introduced into the tanning solution by the pine bark, which is responsible for the advantageous acidification of the tanning solutions much valued for making certain kinds of leather, and of decisive importance in the quality, so that tanners add the souring liqour of other vats to encourage the souring of the doubtful one.

Hay is made in very different ways in different countries, and in those where a "spontaneous" heating process is resorted to there seems to be no doubt that certain thermogenic bacteria are concerned. The researches of Böhmer, Dietrich, Fry, Lafar, and others show that here and in the preparation of ensilage we have important fermentation processes which affect the end result.

The whole question of fermentation in hay, and the high temperatures produced in the process, as well as what occurs in straw-stacks under similar conditions, have important theoretical bearings, and we know of bacilli which grow at 70° C.

Probably no other subject in this domain has, however, attained so much importance as the bacteriology of the dairy—the study of the bacteria found in milk, butter, and cheese in their various forms. In all cases of this kind, as in brewing, bread-making, and so on, there are three aspects of the bacteriology of the operations: we have to consider first the bacteria concerned in the normal process; secondly, introduced forms which bring about abnormalities, or "diseases" of the normal operation; and, thirdly, the possible pathogenic bacteria, *i.e.* pathogenic to man, which may lurk in the product.

Of milk especially much has been said as a disease-transmitting medium, and with good reason, as is well known; and if we may accept the statement of a continental authority, who calculated that each time we eat a slice of bread and butter we devour a number of bacteria equal to the population of Europe, we have grounds for demanding information as to what these bacteria are, and what they are doing. And similarly with cheese, every kind of which teems with millions of these minute organisms.

Now I cannot, of course, go into the question of pathogenic bacteria, nor is there time to discuss those forms which bring about undesirable or abnormal processes in the dairy; but I want to call your attention to the splendid field for bacteriological investigation which is being opened up by inquiries into the normal changes utilised in making butter and cheese.

We may pass over the old controversies as to the souring of milk, culminating in Pasteur's discovery of the bacteria of lactic fermentation in 1857–58. Lister in 1877 isolated *Bacterium lactis*. Hueppe in 1884 confirmed his results, and added several other lactic bacteria, and we now know a whole series of forms which can turn milk sour by fermenting its sugar, and this in various ways, as Warington and others have shown. The souring of milk and cream by merely leaving it to stand often led to failure, and the study of this preliminary to butter- and cheese-making is itself a bacteriological question of great importance. We shall not be surprised, therefore, that when, in 1890, Wiegmann proposed to use pure cultures of lactic-acid bacteria for the souring of cream, the plan was at once taken up.

Some years ago Storch found that the peculiar aroma of a good butter was due to a bacterium which he isolated, and Wiegmann has now two forms, or races, one of which develops an exquisite flavour and aroma, but the butter keeps badly, while the other develops less aroma, while the butter keeps better.

According to a recent publication of Conn's, however, this subject has been advanced considerably in America, for they have isolated and distributed to numerous dairies pure cultures of a particular butter-bacillus which develops the famous "June flavour" hitherto only met with in the butter of certain districts during a short season of the year. I am told that this fine-flavoured butter is now prepared constantly in a hundred or more American dairies. Simultaneously with these advances in the manufacture of pure butter with constant flavour, the days of "diseased" butters seem numbered.

Properly considered, the manufacture of cheese is a form of microscopic gardening even more complex and more horticultural in nature than the brewing of beer. From the outset, when the cheesemaker guards and cools his milk till his stock is ready, he is doing all he knows how to do to keep down the growth of the germs introduced into the milk; he then coagulates it, usually with rennet—an enzyme of animals, but also common in plants—and the curd thus prepared is simply treated as a medium on which he grows certain fungi and bacteria, with the needful precautions for favouring their development, protecting them against the inroads of animal and plant pests, and against unsuitable temperature, moisture, access of light, and so on. Having succeeded in growing the right plants on his curd, his art then demands that he shall stop their growth at the critical period, and his cheese is ready for market.

The investigations of Duclaux, Wiegmann, and others on the continent, of Conn in America, and of Lloyd in England, to say nothing of other workers now busy at this subject in various parts of the world, are getting at the particular forms of fungi concerned in so altering the constitution of curd that it becomes the very different article of food we call cheese, and they have even determined to some extent what *rôle* is played by these plants in giving the peculiar odours and flavours to such different cheeses as Camembert, Stilton, and Roquefort. It is known, for instance, that a certain fungus (*Penicillium*) cultivated on bread is purposely added to Roquefort, and that it destroys the lactic and other acids, and so enables certain bacteria in the cheese, hitherto inhibited in their actions by these acids, to set to work and further change the medium, whereas in making Emmenthaler cheese the object is to prevent this fungus thus paving the way for these bacteria. Pammel claims to have discovered a bacillus which gives a peculiar and much-admired clover aroma to certain cheeses, and according to recent statements a definite *Streptococcus* is responsible for the peculiarities of certain Dutch cheeses, and so on. Nevertheless, we are still profoundly ignorant of most of the forms concerned in the ripening of cheese, and every research which throws light on this difficult and complex subject, and so paves the way to rendering uniform and certain this at present most haphazard and risky manufacture will be doing service to the State. Considering that Cohn only discovered that the ripening process is due to bacteria in 1875, and that Duclaux only published his researches on *Tyrothrix* in 1878, we can scarcely be surprised that the interval has not been long enough for the isolation and study of the numerous and curious forms, several hundreds of which are now imperfectly known. Nevertheless, there are signs of advance in various directions, and researches into the mysteries of Roquefort, Gorgonzola, Emmenthaler, and other cheeses are being industriously pursued on the continent. Even as I write this comes the news that Freudenreich has discovered the coccus which causes the ripening of Emmenthaler cheese. It is not impossible that the much more definite results obtained by investigations into the manufacture of the vegetable cheeses of China and Japan will aid bacteriologists in their extremely complex task.

These vegetable cheeses are made by exposing the beans of the leguminous plant *Glycine*—termed soja-beans—to bacterial fermentations in warm cellars, either after preliminary decomposition by certain mould-fungi, or without this. The processes vary considerably, and several different kinds of bean-cheeses are made, and known by special names. They all depend on the peculiar decompositions of the tissues of the cotyledons of the soja-bean, which contain 35 to 40 per cent. of proteids and large quantities of fats. The softened beans are first rendered mouldy, and the interpenetrating hyphæ render the contents accessible to certain bacteria, which peptonise and otherwise alter them.

Here, however, I must bring this subject to a close, and time will not permit of more than the mere mention of the vinegar fermentation, to which Mr. Adrian Brown has lately contributed valuable knowledge, of the preparation of soy, a brine extract of mouldy and fermented soja-beans, of bread-making, and other equally interesting cases.

When the idea of parasitism was once rendered definite, as it was by De Bary's work, and the fundamental distinction between a parasite and a saprophyte had been made clear, it soon became evident that some distinction must be made between *obligate facultative parasites* and *saprophytes* respectively; but when De Bary proposed the adoption of these terms of Van Tieghem's he can hardly have contemplated that they would be abused as they have been, and was clearly alive to the existence of transitions which we now know to be so numerous and so gradual in character that we can no longer define any such physiological groups.

Twenty years ago *Penicillium* and *Mucor* would have been regarded as saprophytes of the most obligate type, but we now know that under certain circumstances these fungi can become parasites; and the border-land between facultative parasites and saprophytes on the one hand, and between the former and true parasites on the other, can no longer be recognised.

In 1866 the germ of an idea was sown which has taken deep root and extended very widely. De Bary pointed out that in the case of lichens we have either a fungus parasite on an alga, or certain organisms hitherto accepted as algæ are merely incomplete forms. In 1868 Schwedendener declared the lichen to be a compound organism.

In 1879, in his celebrated lecture, De Bary definitely launched the new hypothesis, and brought together the facts which warranted his disturbance of the serenity of those unprepared to accept so startling a new notion as *Symbiosis*.

The word itself, in the form "Symbiotismus," is due to Frank, who, in an admirable paper on the biology of the thallus of certain lichens, very clearly set forth the existence of various stages of life in common.

This paper has been too much overlooked; but its existence is the more noteworthy from its being in the same number of the "Beiträge zur Biologie"—which we owe to Cohn, the founder of scientific bacteriology—in which Koch's remarkable paper on Anthrax occurs.

The details of these matters are now principally of historical interest; we now know that lichens are dual organisms, composed of various algæ, symbiotic with ascomycetes and even basidiomycetes, and, as Massee has shown, even gastromycetes. The soil contains also bacterio-lichens. The point for our consideration is rather that botanists were now awakened to a new biological idea—viz. that a fungus may be in such nicely balanced relationships with the host from which it derives its supplies as to afford some advantage in return, whence we must look upon the limited liability company formed by the two symbionts as a better business concern than either of the plants could establish for itself—a case, in fact, where union is strength. Symbiosis, consequently, is now understood to be of advantage to both the symbionts, and not to one only, as is the case in parasitism, or, to use Vuillemin's term, *Antibiosis*.

In 1841 an English botanist, Edwin Lees, discovered the existence of "a hirsuture that appears like a byssoid fungus" on the roots of *Monotropa*, and observed that the hyphæ linked the roots to those of a beech; he regarded the fungus as conveying nutriment from the latter to the former, and as an essential constituent of the *Monotropa*. This discovery was published in the now defunct "Phytologist" for December 1841, and was unearthed by Oliver and by Dr. Dyer, of Kew. This is apparently the first observation of a mycorhiza yet recorded, and, although the naturalists referred to did not understand the full significance of Lees' find, several of them made excellent guesses as to the meaning of the phenomenon. As Dr. Dyer points out, it disposes of Wahrlich's claim that Schleiden (1842) first discovered mycorhiza, as well as of Woronin's contention that the priority is due to Kamienski, though the latter (1881–82) probably was the first to clearly indicate that we have here a case of symbiosis, and thus anticipated Frank's generalisation in 1885.

Kamienski and Frank, followed by numerous other observers, among whom Oliver and Groom are to be mentioned, have now shown that the peculiar type of symbiosis expressed in this intimate union of fungus-hyphæ with the living cells of the roots of trees and other plants in soils which abound in vegetable remains—*e.g.* leaf-mould, moors, &c.—is very common.

In the humus of forests we find the roots of beeches and other *Cupuliferæ*, willows, pines, and so forth, clothed with a dense mantle of hyphæ and swollen into coral-like masses of mycorhiza; in similar soils, and in moorlands which abound in the slowly decomposing root-fibres and other vegetable remains so characteristic of these soils, the roots of orchids, heaths, gentians, &c., are similarly provided with fungi, the hyphæ of which penetrate further into the tissues, and even send haustoria into the living cells, but without injuring them.

As observations multiplied it became clear that the mycorhiza, or fungus-root, was not to be dismissed as a mere case of roots affected by parasites, but that a symbiotic union, comparable to that of the lichens, exists; and that we must assume that both the tree and the fungus derive some benefit from the connection.

Pfeffer, in 1877, suggested that the deficiency of root-hairs observed in orchids might be explained by the fungus-hyphæ playing the part of these organs, and taking up materials from the soil which they then handed on to the roots. He is quite clear on the subject, and recognises the symbiosis definitely, comparing it with other cases of symbiosis indicated by De Bary.

Frank stated that, as the results of experiments, seedling forest-trees cannot be grown in sterilised soil, where their roots are prevented from forming mycorhiza, and concluded that the fungus conveys to the roots organic materials, which it obtains by breaking down the leaf-mould and decaying plant-remains, together with water and minerals from the soil, and plays especially the part of a nitrogen-catching apparatus. In return for this important service the root pays a tax to the fungus by sparing it certain of its tissue contents, and no doubt can well afford to do so.

It appears that the mycorhiza is only formed where humus or vegetable-mould abounds. In sandy soils the roots bear root-hairs, as usual, and it is now clear that, while mycorhiza is a far more general phenomenon than was previously supposed, it is not essential for all the roots, nor even under all circumstances for any of them.

Probably what really happens is this. Trees and other plants with normal roots and root-hairs, when growing in ordinary soil, can adapt their roots to life in a soil heavily charged with humus only by contracting the symbiotic association with the fungus and paying the tax demanded by the latter in return for its supplies and services. If this adaptation is impossible, and no other suitable variation is evolved, such trees cannot grow in such soils.

In certain cases—*e.g.* ground orchids, *Monotropa*, various *Ericaceæ*, &c.—it would seem that the plant is unable to grow in other than humus soils, and always forms mycorhiza.

Much further we cannot at present go, but it is evident that various different grades of symbiosis exist in these mycorhizas.

In the first place, there are several different fungi concerned—those on cupuliferæ and pines, apparently mostly *Tuberaceæ* and *Gasteromycetes*, and allied forms, being different from those in orchids, some at least of which appear to be *Nectrias* or related genera.

The physiological relations of the root to the fungus must be different in details in the case of non-green, purely saprophytic plants, like *Neottia*, *Monotropa*, &c., and in that of the green plants like *Erica*, *Fagus*, *Pinus*, &c.

It is well known that ordinary green plants cannot utilise vegetable débris directly, whereas trees in forests appear to do so; this in appearance only, however, for the fungi, yeasts, and bacteria there abounding are actively decomposing the leaves and other remains.

Now it is possible that the mycorhiza theory is not applicable in all cases, and that, sometimes, what happens is this. The trees, once well established, make so good a fight that in spite of the leaf-decomposing fungi attacking their roots parasitically, or merely ensconcing themselves in the dead primary cortex as it is sloughed, they manage to keep going and to obtain such shares of the nitrates and other products due to the fungus-action as satisfy their needs. But although there may be something to be said for this view as regards a few forest-trees, it is not easy to see how it would apply to the non-assimilating humus-plants like *Neottia*, *Monotropa*, &c., and we may probably regard the two sets of cases as standing or falling together.

No treatment of this subject would be complete without reference to those obscure cases of symbiosis—as we must regard them—between certain algæ which occur in the cavities of the leaves of *Azolla* and in *Gunnera*, and those found in the intercellular spaces of cycad-roots. When we know more of the physiology of these blue-green algæ, it may be possible to explain these puzzles, but at present they are mysterious curiosities.

A class of pseudo-symbiotic organisms is being more and more brought into the foreground where the combined action of two symbionts results in death or injury to a third plant, whereas each symbiont alone is harmless, or comparatively so.

Some time ago Vuillemin showed that a disease in olives results from the invasion of a bacillus (*B. oleæ*), which, however, can only obtain its way in the tissues through the passages driven by the hyphæ of a fungus (*Chætophoma*). The resulting injury is a sort of burr. Vuillemin has this year observed the same bacillus and fungus in the canker burrs of the ash, and so confirms Noack's statement to the same effect.

Among many similar cases, well worth further attention, the invasion of potato-tubers by bacteria, which make their way down the decaying hyphæ of pioneer fungi, may be noted. I have also seen tomatoes infected by these means, and have facts showing that many bacteria which quicken the rotting of wood are thus led into the tissues by fungi.

Probably no subject in the whole domain of cryptogamic botany has wider bearings on agricultural science than the study of the flora and changes on and in manure and soil.

As vegetable physiology and agricultural science progressed, it became more and more of primary importance that we should learn what manure is composed of, what changes it undergoes in the soil, and what the roots of plants do with it. Chemistry did much to solve some of the earlier problems, but it soon became evident that it only raised new questions which it could not solve; and it was not till the sequence of changes induced by the successive growths of *Mucor*, *Pilobolus*, *Coprinus*, *Ascobolus*, and other moulds and fungi of various sorts, followed by bacteria and yeasts, began to be understood, that anything approaching a coherent account of the complex phenomena going on in soil or in a manure-heap could be attempted. Not that all the difficulties have been solved even now, but we are at least able to trace some very important chains of occurrences which throw light on many hitherto obscure matters going on in the field.

Since Pasteur in 1862, and Van Tieghem in 1864, showed that certain bacteria are concerned in converting urea to ammonium carbonate, much has been learnt, and we now know from the investigations of Miquel, Jaksch, Leube, and others that numerous urea-bacteria exist; and Miquel, in 1890, isolated an extremely unstable enzyme—urase—which converts sterile urea to ammonium carbonate very rapidly, a discovery of considerable interest, as it was one of the first examples of this class of bodies to be examined; and when we reflect on the enormous quantities of urea which have to be destroyed daily, and that fresh urine is in effect a poison to the roots of higher plants, some idea of the importance of these urea-bacteria is obtained. The necessity for preventing the losses of this volatile ammonia by fixing it in the soil and presenting it to the action of the nitrifying organisms is also obvious.

Winogradsky's classical isolation and cultivation of bacteria which take up these ammonia compounds and oxidise them to nitrous and to nitric acids in the soil, may be quoted as further instances of the bearing of bacteriological work on this department of science, as explaining not only the origin of nitre-beds and deposits, but also the way the ammonia compounds fixed by the soil in the neighbourhood of the root-hairs are nitrified and so rendered directly available to plants.

The theoretical explanation of many questions connected with the washing out of nitrates from fallows, the advantages of autumn and winter sowing, and processes occurring in the upper soil as contrasted with sub-soil, has been rendered much easier by these researches; moreover, as is now well known, they brought to our knowledge a startling instance of the assimilation of carbon-dioxide by these non-green plants—bacteria—which not only take some of the purely inorganic ammonia, but by means of energy set free by its oxidation obtain their carbon also by breaking up the carbonate—a true case of the assimilation of carbon-dioxide by a plant devoid of chlorophyll and without the direct aid of light. Indirectly, it is true, the source of the energy is the light of the sun, because the oxygen employed by these aërobic forms has been liberated by green plants in the last instance; but the case is none the less a startling and important contribution to physiology, and Winogradsky's work, which had been preceded by investigations in England by Warington and others, affords one of the best illustrations I know of the importance of this branch of botanical investigation.

Stutzer and Hartleb's recent publications go to show that the nitrifying organism is a much more highly developed and complex form than has hitherto been suspected; that it can be grown on various media, and exhibits considerable polymorphism—for instance, it can be made to branch, and show the characteristics of a true fungus, statements confirmed to a certain extent and independently by the even more recent work of Rullmann; and it appears that we have much more to learn of the morphology of this widely-spread and interesting plant.

It is impossible to go into the controversy between the observers referred to and Winogradsky, the discoverer of the definite nitrifying organism; but there is one point I must just mention: if Stutzer and Hartleb's details are confirmed we have here the most remarkable case of polymorphism I know of, for they claim characters for their fungus which prevent our putting it into any existing group.

I have for some time insisted on the fact that river-water contains reduced forms of bacteria—*i.e.* forms so starved and so altered by exposure to light, changes of temperature, and the low nutritive value of the river-water, that it is only after prolonged culture in richer food-media under constant conditions that their true nature becomes apparent. Now, Stutzer and Hartleb show that the morphological form of the nitrifying organism can be profoundly altered by just such variations in the conditions as the above, and occurs as a branched mycelial form, as bacilli or bacteria, or as cocci of various dimensions according to conditions.

These observations, and the researches of Zopf, Klebs, and others on variations in form (polymorphism) in other fungi and bacteria, open out a vast field for further work, and must lead to advancements in our knowledge of these puzzling organisms; they also help us to explain many inconsistences in the existing systems of classification of the so-called "species" of bacteria as determined by test-tube cultures.

But the urea bacteria and the nitrifying organisms are by no means the only forms found in manure and soils.

In 1868 Reiset found evidence of a reduction of nitrates in fermenting beet-juices, and in 1873 Schloesing found that free nitrogen escaped in certain soil-fermentations. Further work by Mensel, Deherain, and others led to the suspicion that certain bacteria can undo the work of the nitrifying organisms, and in 1879 Warington showed that both nitrites and nitrates occurred in his soil-fermentations.

In 1886 Gayon and Dupetit put this almost beyond doubt, and in 1891 Giltay and Aberson isolated and cultivated a denitrifying bacterium, capable of completely reducing nitrates with evolution of free nitrogen, provided it is cultivated anaërobically.

Several such forms have now been obtained, the observations of Burri and Stutzer that certain of the commonest bacteria of the alimentary canal— e.g. *B. coli commune*—abounding in fresh manure, are especially active, being particularly suggestive. You will thus notice that we have now a sketch of the whole of the down-grade part of the cycle of organic nitrogen in nature; it only needs supplementing by the history of the fixation of free nitrogen from the atmosphere by leguminous plants and certain soil-organisms to complete the sketch.

As is well known from investigations in which Eriksson, Woronin, Frank, Prazmowski, and others, including myself, have taken part, the nodules on the roots of leguminous plants contain a fungus—the morphological nature of which is in dispute—living in symbiotic union with the protoplasm of the cells. Hellriegel and Wilfarth showed in 1888-90 that, provided the root-nodules are present, these leguminous plants fix the free nitrogen of the atmosphere; and Laurent and Schloesing put this beyond all doubt in 1892 by demonstrating that a closed atmosphere in which *Leguminosæ* grow loses nitrogen in proportion as the plants gain it. Meanwhile Schulz Lupitz had shown that agricultural land poor in nitrogen can be made to accumulate it in paying quantities by growing lupines on it, and quite recently pure cultures of the organism of the nodules have been placed on the market under the unfortunate name *Nitragin*. It is claimed that these organisms can be readily used in practice to inoculate the seeds or soil.

Kossowitsch in 1894 showed that certain symbiotic unions of algæ with bacteria are also capable of fixing nitrogen; and Winogradsky declares that there exists in the soil a bacterium which, provided it is kept protected from oxygen by aërobic soil organisms, can itself do this. We are quite unaware of the mechanisms here concerned; but in all cases it appears certain that active destruction of carbohydrates is an essential condition, and we can only assume that the nitrogen is forced into synthetic union by means of energy derived from this destruction. Here, then, we have a glimpse of the up-grade part of the cycle of nitrogen in nature, the importance of which to agriculture cannot be overrated. As to the theoretical bearings of the matter, we are still much in the dark, and can only anxiously await the results of further investigations into the nature of the peculiar fermentations and their products going on in these nodules. I now want to draw your attention to a bearing of the above discoveries concerning denitrifying bacteria on some agricultural and horticultural questions.

It is well known that a gardener eschews the use of fresh manure. Why is this? The most obvious reply might seem to be, because the ammonia compounds and other nitrogenous constituents in such manure are not directly useful, or are even harmful to the roots of the plants. Some recent researches suggest that the matter is more complex than this.

It has not unfrequently happened that a farmer, finding himself short of stable-manure, has made up the deficit by adding some such artificial manure as Chili saltpetre, his argument running somewhat as follows:—Both are good nitrogenous manures, the one acting slowly, the other rapidly, so that a mixture of both should be better than either alone. The results have disappointed him, and numerous experiments in Norfolk, as I am informed by Mr. Wood, and in the North of England, as Dr. Somerville assures me, have shown that most disastrous results ensue if such mixtures are used, whereas if the farmyard manure is employed at first—the "shorter" the better—and the nitrates applied later on as a "top-dressing," excellent crops follow. The explanation seems to come from some recent experiments by Wagner, Maercker, Burri and Stutzer, and others. The farmyard manure, especially if fresh, so abounds in denitrifying bacteria that they destroy the nitrates rapidly and completely, free nitrogen escaping. Curiously enough, a very active denitrifying bacillus was found on straw, and we know that straw abounds in such manures.

I did not intend to go so far into agricultural details as this, but it was impossible to resist these illustrations of the splendid field of mycological research which here lies before us.

Nor can I avoid instancing at least one more example of the organisms at work in manure. We all know what enormous quantities of cellulose are manufactured daily, and even hourly, by the activity of green leaves; and when we reflect on the millions of tons of dead-wood, straw, fallen leaves, roots, &c., which would accumulate every year if not destroyed, we see at once how important is the scavenging action of the moulds and bacteria which gradually reduce these to carbon-dioxide and water, setting these gases free to enter once more into the cycle of carbon, oxygen, and hydrogen in nature.

In 1890 Van Senus obtained two bacteria, one an aërobic and the other an anaërobic form, which in symbiotic union were found to excrete an enzyme which dissolved cellulose. Such a cellulose-dissolving enzyme I had myself isolated from the *Botrytis* of the lily disease in 1888. In 1895 Omeliansky, working with river mud, found an anaërobic bacillus which dissolves paper with remarkable rapidity. I can only hint at the importance of these forms in connection with the production of marsh gas in swamps, the question of the digestion of cellulose in herbivorous animals, the manufacture of ensilage, and the processes of "shortening" of manure; and it is clear they have much to do with the destruction of paper, &c., in sewers and refuse-pits. Moreover, their further investigation promises a rich harvest of results in explanation of the rotting of stored tubers, certain diseases of plants, and several theoretical questions concerning anaërobism, butyric fermentation, and, possibly, that extremely difficult question on which Mr. Gardiner has done such excellent work, the nature of the various celluloses and constituents of the cell-wall.

I now turn to the subject of fungus epidemics, of world-wide interest, if only because the annual losses to agriculture due to epidemic diseases of plants amount to millions of pounds sterling.

The history of wheat-rust can be traced to Genesis, and at least five references to it exist in the Old Testament. The Greeks were familiar with it, and the Romans had a special deity and special ceremonies devoted to it. References can be given to it in old Norman times, and Shakespeare can be quoted as acquainted with it.

According to Loverdo, a law existed in Rouen in 1660, authorising the pulling up of barberry bushes as in some mysterious way connected with rust, and in 1755 the celebrated Massachusetts law was promulgated. Eriksson refers to an English farmer destroying his neighbour's barberry in 1720.

The words *Robigo*, *Rubigo*, *Rouille*, *Ruggine*, *Rufus*, and *Rust* comprise a history in themselves, into which, however, we have not time to go, and there are many fascinating points in the history of wheat-rust which must be passed over.

Felice Fontana in 1767 probably made the first scientific investigation of rust; he distinguished the uredo- and puccinia-stages under other names, and even thought of them as rootless plants exhausting the wheat; in this, and his conviction that no remedy was possible until a careful study of all phases of the disease had been made, he was far ahead of his times.

Jethro Tull, Marshall, and Withering are the most conspicuous English names in connection with this question and period, and Marshall in 1781-84 experimented intelligently with barberry and wheat inter-planted.

Persoon in 1797 gave the name *Puccinia graminis* to the fungus. In 1805 Sir Joseph Banks described it, and suggested that the germs entered the stomata: he also warned farmers against the use of rusted litter, and made important experiments on the sowing of rusted wheat-grains.

A great discussion on the barberry question followed, in which Banks, De Candolle, Windt, Fries, and others took part, Fries particularly insisting on the difference between *Æcidium berberidis*—a name conferred by Gmelin in 1791—and *Puccinia graminis*.

De Candolle had also distinguished *Uredo rubigo-vera* in 1815, and Schmidt soon after described a third wheat-rust—*Uredo glumarum*.

Matters were at about this stage when Tulasne confirmed the statement of Henslow—one of my predecessors in Cambridge—that the uredo- and puccinia-stages really belong to the same fungus, and are not, as Unger asserted, mixed species.

Then came De Bary and his classical investigation of the whole question in 1860-64. He proved that the *sporidia* of some Uredineæ (e.g. *Coleosporium*) will not infect the plant which bears the spores, and that the æcidia of certain other forms are only stages in the life-history of species of *Uromyces* and *Puccinia*.

In 1864 De Bary attacked the question of wheat-rust, and by means of numerous sowings of the teleutospores on barberry proved beyond doubt that they bring about its infection.

But De Bary did more. For the first time in history he saw the entrance of the infecting tube and the beginning of its growth in the tissues. In 1865 he demonstrated in the same faultless way the infection of the cereal by means of the æcidiospores, and showed that *P. rubigo-vera* alternates on Boragineæ as *Æc. asperifolii*, while *P. coronata*, separated by Corda in 1837, does the same as *Æc. Rhamni* on *Rhamnus*.

Thus was discovered the astounding phenomenon of *Heterœcism*, introducing a new idea into science and clearing up mysteries right and left.

During the next twenty-five years the number of heterœcious forms has risen to about seventy, including Woronin's recent discovery of this phenomenon in an ascomycete—*Sclerotinia heterœcia*.

About 1890 the rust question entered on a new phase. In Australia, India, Sweden, Germany, and America especially, active commissions, inquiries, and experiments were set on foot, and amid some confusion of meaning among some of those concerned much knowledge has resulted from the investigations of Plowright and Soppitt in England; Barclay in India; Cobb, Anderson, and McAlpine in Australia; Arthur, Bolley, Smith Ellis, Galloway, Farlow, Harper, and others in the United States; Dietel, Klebahn, Sorauer, and others in Germany; Rostrup in Denmark; and especially from the continued and indefatigable researches of Eriksson and Henning in Sweden. This renewed work has resulted in the complete confirmation of De Bary's results, but with the further discovery that our four common cereals are attacked by no less than ten different forms of rust belonging to five separate species or "form-species," and with several physiological varieties, and capable of infecting the barberry. Some of these are strictly confined to one or other of the four common cereals, others can infect two or more of them, and yet others can infect various of our common wild grasses as well.

The fact that what has usually gone by the name of *Puccinia graminis* is an aggregate of several species is in itself startling enough, but this was not unexpected; the demonstration that varietal forms exist so specially adapted to their host that, although no morphological differences can be detected between them, they cannot be transferred from one cereal to another, points, however, to physiological variation of a kind met with among bacteria and yeasts, but hitherto unsuspected in these higher parasitic fungi. It now appears that we must be prepared for similar specialisation of varietal forms among *Ustilagineæ* as well as among other *Uredineæ*, as follows from the results obtained by Kellermann and Swingle in America, by Klebahn, Tubeuf, and others in Germany, and by Plowright and Soppitt in England.

Not less remarkable is the conviction that among the many different pedigree varieties of wheat, some are more susceptible to attacks of rust than others. This had often been asserted in general terms, but the extensive observations of Cobb in Australia, and the even more extensive and exact experiments of Eriksson in Sweden, seem to put the matter beyond doubt.

Of course attempts have been made to account for these differences in predisposition to the attacks of wheat-rust.

N. A. Cobb, who has done much for the investigation of Australian wheat-rusts, regards the different susceptibility to rust as due to mechanical causes, and seeks to explain it by the difference in thickness of the cell-walls on the upper and lower leaf-surfaces offering different resistance to the outbreak of the spore-clusters; the average number of stomata per square millimetre differing in the different sorts of grain, influencing the predisposition to infection; the presence of waxy bloom affording a protection, and so on.

Eriksson and Henning have made a critical examination of Cobb's mechanical theory, and show that, for Sweden at any rate, the conclusions of the Australian investigator cannot be confirmed.

Nevertheless, the problem remains. As matter of fact, different sorts of wheat, of oats, of barley, and of rye are susceptible to their particular rusts in different degrees, and the question is, Why? Some complex physiological causes must be at the bottom of it.

Sorauer pointed out in 1880 that every change of vegetative factors induces differences in composition and form of a plant, and therefore alters the predisposition of each individual and variety; and this applies to the fungus as well as to the host.

De Bary's proof, in 1886, that a Peziza succeeds in being a parasite only after saprophytic culture to a strong mycelium, that its form is altered thereby, and that probably a poison is excreted, throws side-lights on the same question; while I myself showed that similar events occur in the case of the lily disease.

Reinhardt, in 1892, showed that the apical growth of a Peziza is disturbed and interrupted if the culture solution is concentrated by evaporation or diluted; and Büsgen, in 1893, showed that *Botrytis cinerea* excretes poison at the tips of the hyphæ, confirming my results with the lily disease in 1888, and that a similar excretion occurs in rust-fungi.

De Bary had also shown, in 1886, that the water-contents of the infected plant influence the matter; and I may remark that we have here also to consider the case of *Botrytis* attacking chrysanthemums, &c., in autumn, with respect to the chilling of the plant, which lowers the vitality of the cells and causes plasmolysis, as well as the fact that cold increases the germinating capacity of spores, as Eriksson showed.

I discussed these points at some length a few years ago in the Croonian Lecture to the Royal Society, and it now remains to see if any further gleams of light can be found in the progress of discoveries during recent years.

You are all no doubt familiar with Pfeffer's beautiful work on chemotaxis, and with the even more fascinating experiments of Engelmann, which prove that bacteria will congregate in the neighbourhood of an algal cell evolving oxygen.

When Pfeffer took the matter up in 1883, he was interested in the question as to the stimulating action of various bodies on mobile organisms, for he found that many motile antherozoids, zoospores, bacteria, &c., when free to move in a liquid, are vigorously attracted towards a point whence a given chemical substance is diffusing.

Pfeffer's problems had nothing to do with those of Engelmann; he was concerned, not with the proof of oxygen evolution or the movements of bacteria as evidence of the presence of that element, but with a fundamental question of stimulation to movement in general.

Pfeffer found that the attractive power of different chemical substances varies according to the organism, and according to the substance and its concentration. He also showed that various other bodies besides oxygen thus attract bacteria—*e.g.* peptone dextrose, potassium salts, &c. These experiments are by no means difficult to repeat, and are now employed in our laboratories.

During the course of several years not only were these facts confirmed, but it was also shown that this remarkable attraction—chemical attraction, or "*chemotaxis*"—is a very general phenomenon.

Pfeffer had already shown that swarmspores of the fungus *Saprolegnia* are powerfully attracted towards the muscles of a fly's leg placed in the water in which they are swimming about, and pointed out that in many cases where the hyphæ of fungi suddenly and sharply bend out of their original course to enter the body of a plant or animal, the cause of the bending lies in a powerful "*chemotropic*" action due to the attraction of some substance escaping from the body.

This idea of an attractive action between the living substance of two organisms growing in close proximity was not entirely new—it was, so to speak, in the air—*e.g.* the fusions of mycelial cross-connections and clamp-organs, and of the spores of *Tilletia*, *Entyloma*, &c. One of the most striking examples is afforded by Kihlmann's demonstration of the parasitism of *Melanospora* on *Isaria*, where he states that some attractive action exists. In 1882 I had myself seen zoospores of *Pythium* suddenly dart on to the cut surface of a bean-stem, and there fix themselves. But it is due to Pfeffer and his pupil Miyoshi to state that they were the first to demonstrate these matters clearly.

To understand the important consequences which followed, I must now refer to another series of discoveries.

When a spore of a parasitic fungus settles on a plant, it frequently behaves as follows. The spore germinates and forms a slender tube of delicate consistence, blunt at the end and containing colourless protoplasm. De Bary long ago showed that such a tube—the germinal hypha—only grows for a short time along the surface of the organ, and its tip soon bends down and enters the plant, either through one of the stomata or by boring its way directly through the cell-walls. Several observers, and among others myself, remarked how the phenomena suggested that the end of the tube is attracted in some way and by some force which brings its tip out of the previous direction, and De Bary even threw out the hint that this attraction might be due to some chemical substance excreted by the host-plant. I myself showed that the condition of the attacked plant affected the ease with which the tube penetrates the cell-walls, and that the actual boring of the cell-walls is due to a solvent enzyme secreted by the tip of the fungus, and in clearly demonstrating this excretion of an enzyme capable of dissolving cellulose carried a step further what was so far known, principally from De Bary's researches, as to this process. In 1892, Reinhardt showed that the tips of hyphæ curve over towards spores they are about to attack, and found that sugar-gelatine of greater strength attracts them from the same medium with a smaller proportion of sugar.

Miyoshi then showed, in 1894, that if a leaf is injected with a substance such as ammonium-chloride, dextrine, or cane-sugar, all substances capable of exerting chemotropic attraction on fungus-hyphæ, and spores of a fungus then sown on it which is *not parasitic*, the hyphæ of the fungus penetrate the stomata and behave exactly as if the fungus were a true parasite.

This astounding result throws a clear light on many known cases of fungi which are, as a rule, *not* parasitic, becoming so when the host-plant is in an abnormal condition—*e.g.* the entry of species of *Botrytis* into living tissues when the weather is cold and damp and the light dull; the entry of *Mucor* into various fruits, such as tomatoes, apples, pears, &c., when the hyphæ meet with a slight crack or wound, through which the juices are exposed. Nay, I venture to suggest that it is even exceedingly probable that the rapid infection of potato-leaves in damp weather in July is not merely traceable to the favouring effect of the moisture on the fungus, but that the state of super-saturation of the cell-walls of the potato leaf, the tissues of which are now unduly filled with water and dissolved sugars, &c., owing to the dull light and diminished transpiration, is the primary factor which determines the easy victory of the parasite, and I suggested some time ago that the suppressed life of *Ustilagineæ*, in the stems of grasses, is due to the want of particular carbo-hydrates in the vegetative tissues there, but which are present in the grain.

Miyoshi, in 1895, carried to proof the demonstration that a fungus-hypha is really so attracted by substances on the other side of a membrane, and that its tip pierces the latter; for the hyphæ were made to grow through films of artificial cellulose, of collodion, of cellulose impregnated with paraffin, of parchment-paper, cork, wood, and even the chitinous coat of an insect, simply by placing the intact films on gelatine impregnated with the attracting substance, and laying the spores on the opposite side of the membrane.

Hyphæ so separated by similar membranes from gelatine to which the attracting substance was not added, did not pierce the membranes, whence we may conclude that it is really the substance referred to which incites the hyphæ to penetration.

Now, obviously, this is a point of the highest importance in the theory of parasitism and parasitic diseases, because it suggests at once that in the varying conditions of the cells, the contents of which are separated only by membranous walls from the fungus-hyphæ, whose entrance means ruin and destruction, there may be found circumstances which sometimes favour and sometimes disfavour the entrance of the hyphæ; and it is at least a remarkable fact that some of the substances which experiments prove to be highly attractive to such hyphæ—*e.g.* sugars, the sap of plums, phosphates, nitrates, &c.—are just the substances found in plants, and the discovery that the action depends on the nature of the substance as well as on the kind of fungus, and is affected by its concentration, the temperature, and other circumstances, only confirms us in this idea.

Moreover, there are substances which repel, instead of attracting the hyphæ.

Is it not, then, natural to conclude that the differences in behaviour of different parasites towards different host-plants, and towards the same host-plant under different conditions, probably depend on the chemotropic irritability of the hyphæ towards the substances formed in the cells on the other side of the membranous cell-walls? And when, as often happens, the effusion of substances such as the cells contain to the exterior is facilitated by over-distension and super-saturation, or by actual wounds, we cannot be surprised at the consequences when a fungus, hitherto unable to enter the plant, suddenly does so.

In spite of all the progress made towards an explanation of the origin and course of an epidemic of rust, however, one serious inconsistency has always puzzled men who have worked with it in the open and on a large scale. This inconsistency concerns the outbreaks of epidemics over large areas, at periods, and within intervals, which do not agree with the weather records and the described biological facts. We know, speaking generally, the conditions of germination of the spores, we know how long infection requires, and the latent period is known: we know much as to the conditions which favour or disfavour the fungus mycelium in the tissues, and, nevertheless, an outbreak of disease over large areas sometimes occurs under conditions which appear quite inconsistent with this knowledge.

During his six years' study of the wheat rusts Eriksson was so impressed with these difficulties that he has lately committed himself to an hypothesis which may perhaps crystallise the ideas which have floated in the minds of several who have been puzzled by these matters.

The facts which seem to have finally impelled Eriksson to his hypothesis were those of the distribution of the wild rusts and grasses. Having learnt which grasses could infect the wheat, oat, barley, and rye respectively, he found cases of epidemics occurring where it was impossible to fit in the facts with the view that spores had been transferred from these grasses within the period required for infection and development of the disease spots. Again, seasons occurred when all the conditions pointed to the probability of a serious outbreak of rust, and no such epidemic occurred. Further, experiments were made in which cereals of varieties known to be susceptible to given rusts were planted in close vicinity to grasses infected with such rusts, and, nevertheless, in seasons eminently suitable for the outbreak of this particular rust on these particular cereals none appeared, or so little that it was impossible to explain the outbreaks of this same rust on this cereal elsewhere, during that season, as due to direct infection from the surrounding grasses.

More and more it became evident that the infective capacity of the rusted grasses is small, and confined to restricted areas, and that the outbreaks in certain seasons—rust-years—must be due to something other than wind-borne spores distributed by gales over the district.

Three hypotheses can be suggested to account for the non-spreading of the disease on to susceptible cereals—(1) Indisposition to germinate on the part of the spores; (2) unfavourable weather for germination; (3) some structural peculiarities of the leaves on which the spores fell, of such a nature that infection was prevented.

The results of many experiments showed that, as matter of fact, the spores are often very obstinate, and refuse to germinate even when the weather is apparently favourable, and Eriksson discovered during these experiments that cooling the ripe spores on ice increased their germinating power. Neither of the other two hypotheses mentioned could be brought into agreement with the results, however.

The conclusion was thus arrived at that an outbreak of rust cannot always be referred directly to the normal germination and infection of wind-borne spores from neighbouring centres of infection.

In some patches of extremely susceptible cereals, the disease appeared simultaneously on plants isolated from all perceptible sources of infection, and on plants not thus protected; the date of outbreak in these cases—reckoned from the sowing of the grain—was far too late to be explained by direct infection from spores on the soil, or on the grain sown. Experiments demonstrated that if such spores had been there, and germinal tubes formed as usual, the disease would have shown itself much earlier.

These and numerous other inconsistencies drove Eriksson to look for an "internal source of infection," in spite of the improbability of any such existing, and of its apparent incompatibility with scientific theory since De Bary's time.

Two methods were pursued. In one each plant of the cereal was enclosed from the beginning in a long glass tube, stuffed with cotton-wool above and below, and so carefully protected against infection from wind-blown spores that we may accept forthwith the improbability of such infection.

Notwithstanding these precautions, the cereal was rusted at the same time as its unprotected neighbours, and equally badly.

Granting the accuracy of the experiments, only two explanations seem to suggest themselves. Either (1) winter-spores attached to the grain had germinated and infected the young seedling—a not impossible event, since several observers have found spore-bearing mycelia in the pericarp of the ripe grains, and we know these spores can conserve their germinating power for months; or (2) the infective material had been handed down to the embryo from the parent plant—an almost inconceivable hypothesis.

To answer this question Eriksson protected his seed-plants from external infection, and sowed the grains in sterilised soil in specially constructed greenhouses, through which the air can only pass *viâ* cotton-wool filters. Between the double-glass windows water was allowed to stream, and the plants thus kept cool. Some of these protected plants became rusted.

Before we draw any conclusions from such difficult experiments as the above, let us see the results of microscopic examination.

Reference has already been made to the mycelium and spores in the tissues of the pericarp of the grain; no trace could be, or ever has been, detected in the endosperm or embryo. In some cases the seedlings, four to eight weeks old, showed the first uredo-pustules on their leaves, and the mycelium but no spores could be detected in the seed-coats.

The tissues of the leaf, in the neighbourhood of young uredo-pustules, frequently showed curious clumps of protoplasm in the cells, either free in the cell-cavity, or attached to the primordial utricle, and looking like haustoria. Eriksson assumes that we have here the key to the puzzle; he regards these "plasmatic corpuscles" as the protoplasm of the fungus which, after leading a dormant life commingled symbiotically with the living protoplasm of the cell, is now gaining the upper hand and beginning to form a dominant mycelium.

We are therefore to suppose that when the spores of rust, even if of the right variety, alight on the tissues of a wheat-plant, it is a matter decided by external and internal conditions whether the germ-tubes forthwith infect the plant and grow out into a dominant, parasitic, sporiferous mycelium, as we know they usually do, or simply manage to infect the cells with enough protoplasm to live a latent symbiotic life for weeks—or even months—as a *Mycoplasma*, which may, under favourable circumstances, gain the upper hand, and grow out in the form of a mycelium.

This is a startling hypothesis, and brings us to the most advanced point along this line of biological speculation. We must distinguish sharply and clearly between such a view, which is by no means inconsistent with all we know of parasites, so far as the dormant mycelium goes, and all the hazy, mystical suggestions as to "infective substance" and so forth, which were so freely flung about at the beginning of this epoch, and which De Bary's strictly scientific methods put down so firmly.

The idea of symbiosis is now comparatively old, and there are many cases of dormant life now well established. Even the astounding notion of blended protoplasms can no longer be regarded as new. I need only remind you of Cornu's *Rozella*, which invades the thallus of *Saprolegnia* and *Woronina* in *Vaucheria*, the protoplasm of the two organisms apparently blending and living a common life for some time before the true nature of the parasite manifests itself. Eriksson has avowedly been influenced by these and other cases among the *Chrytridiaceæ*. That the remarkable intra-cellular fusions of *Plasmodiophora* and the now well-established symbiosis of the organism of the leguminous root-nodules have also had their influence on his work may well be assumed, and I think we may trace also the effects of our knowledge of the latent life of *Ustilago* during the vegetative period of the attacked cereal.

But there are other cases which prevent our casting aside as impossible the view that Eriksson has put forward.

I showed some years ago that the mycelium of the *Botrytis* of the lily disease can lie dormant for some time in the cell-walls, and I have observations showing that other forms of *Botrytis* which attack roses and chrysanthemums only gain the upper hand when the cold autumn nights so chill the attacked cells that they succumb; the mycelium was there long before, but so long as the cells were active no progress could be made, and only when the plasmolysed chilled cells exude their sap can the mycelium advance.

Many cases of similarly dormant mycelia appear to exist in those cortex and cambium diseases which result in the production of cankers—e.g. *Nectria ditissima* and *Peziza Willkommii*, and Tubeuf's experiments with *Gymnosporangium* are even more suggestive. Tubeuf found that if *G. clavariæforme* is sown on hawthorn seedlings the fungus forms yellow spots and induces marked hypertrophy, and normal spermogonia and æcidia—*Roestelia lacerata*—are developed; but if *Pyrus Acuparia* is used as the host, no yellow spots or hypertrophy result, though a mycelium is formed and will even produce a few starved spermogonia. On allied species of *Pyrus* the fungus may even succeed in forming a few poorly developed æcidia. But on the quince the fungus only just succeeds in establishing an infecting mycelium, and soon dies; and Wagner describes similar events with fungi on *Stellaria*.

These cases point to a struggle between the protoplasm of the cells of the different hosts, and of the fungus respectively: sometimes one wins, sometimes the other. The following cases are also suggestive. De Bary found that the germinal hyphæ of *Peronospora pygmæa*, which is parasitic on *Anemone*, will penetrate the tissues of *Ranunculus Ficaria*, but cannot maintain its hold, and the mycelium soon succumbs and dies.

Still more remarkable and to the point is the following case. Soppitt and Plowright in England, and Klebahn and others on the continent, have gradually unravelled a curious case of heterœcism and specialised parasitism among certain *Puccinias* found on *Smilax*, *Convallaria*, *Paris*, and *Digraphis*. The story is too long to recount in detail, but the *Puccinia-spores* from *Phalaris* were found by Klebahn to refuse to infect *Polygonatum* leaves successfully, though they readily infect the allied *Convallaria*. Close investigation showed, however, that, although the sporidia failed to develop a mycelium in the *Polygonatum* leaves, they really penetrate the cells, and the delicate germ-tube is killed off by the protoplasm, a red spot marking the place of entrance.

The perennial mycelia of Witches' Brooms, æcidia in Euphorbia, *Taphrina*, and many other perennial mycelia are also cases in point.

It is not my purpose to hold a brief for Eriksson's hypothesis, but I may point out that it is in no way contradictory to the facts already known since De Bary's time. Its most serious aspect is with regard to possible treatment, and it is obviously essential that we should have it tested to the utmost, for it must be remembered that no method of spraying or dusting has been, or apparently can be, devised for cereals; hence the questions as to the existence of really resistent forms, and whether dormant mycelia lurking in their tissues have deceived us in these cases also, require sifting to the bottom. Experience, so far, points to the selection of pedigree wheats and careful cultivation as the first necessities; how far the question of spring *versus* winter wheat aids us is still matter for further experiment; early and late ripening are also concerned. Climate we cannot hope to control, but it remains to be seen—when the facts are known—how far it can be "dodged."

Clearly what is needed, then, is experiments with varieties of wheat under all conditions, and we may congratulate the Australian, Swedish, and United States experimental stations on their preliminary efforts in this direction.

I have only been able to give a mere sketch of this rapidly growing subject, but I think you will agree that we are justified in saying that an epidemic of parasitic fungi depends on the interaction of many factors, congenital variations of the host-plant and topical variations of its cell-contents being probably among the most important; and since we cannot hope to control the variations of the parasite, or the meteorological conditions, it behoves agriculturists to pay more systematic attention to the selection of those varieties of the cereal which are least predisposed to rust.

When we find the annual losses from wheat-rust alone put down at sums varying from 1,000,000*l.* to 20,000,000*l.* in each of the great wheat-growing countries of Europe, India, Australia, the United States, and elsewhere, it strikes one as very remarkable that so little should be done to encourage the scientific investigation of these practical questions. I need hardly say that the establishment and maintenance of a fully equipped laboratory and experimental station does not cost the interest on the smallest of these sums.

It should be also clear that in the further development of our knowledge of the treatment of parasitic diseases of plants the farmer, gardener, and forester can alone supply the experimental evidence which will enable us to put theory to the test in the field, garden, and forest. The botanist, by means of his pure cultures of the fungus, can now show clearly what stage in the life-history of a parasite is vulnerable. In his "microscopic gardens" he can show what antiseptics may be employed, how strong they should be, and when and how they should be employed.

But we must not forget that it is one thing to kill a fungus when grown pure, and another to kill it when growing on or in, or even associated with, other plants, without harming the latter. We may compare the first case to the destruction of weeds on a gravel path, where the antiseptic dressing may be employed lavishly and at any time, because there are no other plants to injure; but it is another matter to kill the same weeds growing in a lawn or a flower-bed, where we have to pay attention to the neighbouring plants.

Experiments in the open, simple in themselves, but conducted intelligently and with due regard to the rigorous demands of science, can alone determine these questions.

Brewers have long known that burning sulphur in the barrels will rid these barrels of the moulds and yeasts growing on their damp beer-soaked sides; and Berkeley saw clearly that sulphur could be applied to the outside of plants on which such fungi as the hop- or grape-mildew, &c., are growing, the critical period being when the spores are germinating, so that the slowly oxidising sulphur should evolve sulphurous acid in just sufficient quantities to destroy the delicate germs without injuring the leaves. And even better results have been attained with Bordeaux mixture.

But it is clear that this can only be done with an intelligent appreciation of the life-history of the fungus, and a knowledge of when the germinating stage is at hand. The successes obtained in France and America with Bordeaux mixture attest this.

It would obviously be absurd to powder sulphur or spray liquids over plants attacked by bunt- or smut-fungi, for we know that the germ-tubes only infect the germinating grain as its first root emerges. Here, as was shown long ago, and especially by the experiments of Hoffmann, Kühn, and De Bary, the practice known as "dressing the grain" must be followed. Knowing that the spores of the fungus are attached to the grain, or to particles of soil around, the efforts must be directed to covering the outside of the grain with an antiseptic which is strong enough to kill the germs but not the grain. If the land is known to be clean, the grain may be immersed in hot water, the temperature being experimentally determined, and high enough to kill the spores but not the wheat, and so on. In these matters also the American stations have done good work.

Neither of these classes of treatment can be adopted, on the other hand, for diseases such as "Finger and Toes," where we have a delicate slime-fungus making its way into the roots already in the soil; but, here again, intelligently devised experiments, such as those of Somerville and Massee, have shown that liming the soil renders it so unfavourable to this disease that it can be coped with.

And similarly with other diseases; the particular methods of dealing with the "damping-off" of seedlings, "dry-rot" in timber, the various diseases of trees, and so on, do and must differ in each case, and the guiding principle must be always the same—having learnt all that can be learnt of the habits of the fungus and of the host, and of the relationships of each to the other and the environment, to see how it is possible to step in at the critical moment and interfere with these relationships in the direction desired by human interests.

The whole matter thus resolves itself into a study of variation —a purely experimental inquiry into complex biological relationships, and it is encouraging to see that this is being understood in the large American and other stations, which are distinguishing themselves by their efforts.

GEOLOGY AT THE BRITISH ASSOCIATION.

THE attendance of British geologists at this meeting of the Association was not large, owing, no doubt, to the counter-attractions of the International Geological Congress in Russia, which drew away also some American geologists who might otherwise have attended. But in spite of this, the section was, on the whole, well attended, and its work was exceptionally arduous and interesting. Though with the exception of the reports of Committees, there were only three papers exclusively British in their scope, the large number of contributions on North American subjects more than made up the deficiency, and gave to this meeting a distinctive character. It is significant of the vitality of earth study on the American side of the Atlantic that the papers should have been, upon the whole, well above the average of other meetings.

Contributions of merely local interest were almost absent, and again and again one was struck by the breadth of view and vigour of generalisation which marked the work submitted to the section. The presence of numerous distinguished Canadian and American geologists, many of the latter coming from the Detroit meeting of the sister Association, furnished an audience ready to grasp and to criticise the views brought forward, and in some instances discussions commenced in Detroit were continued in Toronto. It was, indeed, only the amount of work to be got through which limited the debates. The Glacial papers were exceptionally numerous and full of instruction for the European geologist, while that of Prof. Penck must have had a high interest for American glacialists. It was possible, on the day set apart for these papers, to arrange a series which should span the continent from the Atlantic to the Pacific, and thus to bring before the listener a comprehensive view of the whole bearings of the subject in North America. It is noteworthy that neither in the papers nor in the discussion was there a single dissentient from the view that ice-sheets covering the northern portions of the continent had formed the drifts. This seems to be taken as established beyond all doubt in North America.

Next after the Glacial papers in numbers and importance were those dealing with the Archæan and Palæozoic rocks, these again reflecting the locale of the meeting.

Petrological studies were also fully represented, but Palæontology was comparatively neglected, though a feature of the meeting was the special exhibition of several collections of North American fossils. Those to excite the greatest attention were the beautifully preserved trilobites showing the antennæ and pedal appendages, collected from the Utica Shales of Rome, N.Y., by Dr. C. E. Beecher, and exhibited by Dr. Ami.

The excursions arranged for the geologists were highly successful and most enjoyable. Niagara was visited under the leadership of Mr. G. K. Gilbert, of the U.S. Geological Survey; the inter-glacial deposits of the Don Valley and Scarborough Heights, under the guidance of Prof. A. P. Coleman; while Dr. J. W. Spencer pointed out the main features of the Iroquois Beach, a deformed post-glacial shore-line. Of the long excursion to the Pacific Coast, to which the Canadian Pacific Railway Co. has munificently invited a large party of members, it is at the time of writing too early to speak, but this is being anticipated by all invited as the fitting close of a most enjoyable and memorable meeting. The geologists on this excursion will be under the guidance of their President, Dr. G. M. Dawson, and their Secretary, Prof. A. P. Coleman.

The sectional work of the meeting commenced on Thursday, August 19, with the President's address, an able exposition of the present state of our knowledge respecting the most ancient rocks of Canada, of which, as it has already been given in full in our columns, it is necessary now only to record the favourable reception by the meeting.

Next followed a group of papers treating of the pre-Palæozoic and Palæozoic rocks of North America. Dr. L. W. Bailey described some typical sections in south-western Nova Scotia, in which he showed the succession of the Cambrian rocks of that region and their relation to the granite axis. Dr. R. W. Ells discussed problems in Quebec geology, dealing with the origin of the fundamental gneiss and the Grenville series, and their relations to the Hastings series and the oldest fossiliferous rocks. Mr. J. C. Branner traced the former extension of the Appalachians across Mississippi, Louisana, and Texas beneath the newer rocks.

A most interesting paper at this session was that of Dr. F. D. Adams and Mr. J. T. Nicholson, entitled "Preliminary notice of some experiments on the flow of rocks," in which the authors related how they had placed, accurately-fitting columns of Carrara marble about 4 cm. long by 2 cm. diameter, within specially prepared tubes of Swedish iron, and had subjected them to extreme pressure gradually applied. The rock yielded like a plastic substance, and bulged the enclosing tube. In one experiment the column was reduced from 40 mm. to 21 mm. in height, the deformed marble remaining quite firm and compact, though not so hard as the original rock. When sliced and examined microscopically, polysynthetic twinning of the calcite crystals and other indications of strain were observable. These very suggestive experiments are to be continued, and it is certain that all students of dynamic geology will follow the results with keen attention. At a later session Dr. Adams dealt with the structure and origin of certain rocks of the Laurentian system, his conclusions corresponding with those of European workers on rocks of similar type, since he showed by chemical analysis and microscopic examination that the foliated rocks included two distinct and separable types—the one almost certainly highly altered sediments, and the other of igneous origin.

At Friday's session most of the Glacial papers were read.